

Comment on "Participant Observation and Interviewing: A Comparison"

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I

Insofar as the paper by Becker and Geer says: "Participant observation is a very useful way of collecting data, and here are some illustrations to show how useful we found it in one study," I can take no issue with them. On the contrary, I profited from their discussion of the method and their illustrations of its use.

But, unfortunately, Becker and Geer say a good deal more than that. In their first paragraph they assert that participant observation, by virtue of its intrinsic qualities, "gives us more information about the event under study than data gathered by any other sociological method." And since this is true, "it provides us with a yardstick against which to measure the completeness of data gathered in other ways. . . ."

It is with this assertion, that a given method of collecting data—*any* method—has an inherent superiority over others by virtue of its special qualities and divorced from the nature of the problem studied, that I take sharp issue. The alternative view, and I would have thought this the view most widely accepted by social scientists, is that different kinds of information about man and society are gathered most fully and economically in different ways, and that the problem under investigation properly dictates the methods of investigation. If this is so, then we certainly can use other methods of investigation as "yardsticks" against which to measure the adequacy of participant observation for the collection of certain kinds of data. And my impression is that most of the problems social scientists are studying seem to call for data gathered in other ways than through participant observation. Moreover, most of the problems investigated call for data collected in several different ways, whether in fact they are or not. This view seems to me implied in the commonly used metaphor of the social scientist's "kit of tools" to which he turns to find the methods and techniques most useful to the problem at hand. Becker and Geer's argument sounds to me very much like a doctor arguing that the scalpel is a better instrument than the forceps—and since this is so we must measure the forceps' cutting power against that of the scalpel.

Much of the paper by Becker and Geer is devoted to measuring "the interview" against the yardstick of "participant observation." To make the "contest" between interviewing and participant observation a fair one, the authors make the proviso (footnote 3) that they are employed by men who are equally competent, and who start with equally well formulated problems, "so that they are indeed looking for equivalent kinds of data." I would assume, on the contrary,

that interviewing and participant observation would rarely produce "equivalent" kinds of data, and should not be asked to, but rather produce rather different kinds of data designed to answer quite different kinds of questions about the same general phenomenon. Here again we have Becker and Geer's view of the forceps as a rather poor kind of cutting instrument.

But if I respectfully decline to enter debate on the question of whether the scalpel is a better instrument than the forceps (unless it is rather closely specified "for what")—nevertheless, it may be useful to consider some of the assumptions about the nature of social research out of which such an unreal question can emerge.

II

The first thing that struck me on reading this paper is its oddly parochial view of the range and variety of sociological problems. To state flatly that participant observation "gives us more information about the event under study than . . . any other sociological method" is to assume that all "events" are directly apprehensible by participant observers. But what are some of the "events" that sociologists study? Is a national political campaign such an "event"? Is a long-range shift in interracial attitudes an "event"? Is an important change in medical education and its aggregate of consequences an "event"? Are variations in suicide rates in different social groups and categories an "event"? If we exclude these phenomena from the definition of the term "event" then we exclude most of sociology. If we define "event" broadly enough to include the greater part of what sociologists study, then we find that most of our problems require for their investigation data of kinds that cannot be supplied by the participant observer alone.

But the answer of the participant observation enthusiast, if I read Becker and Geer correctly, would be "that is all very true, but very sad. Many students do require the gathering of data in all kinds of defective and suspect ways, but the closer they approximate to participant observation, and the more frequently they check their findings against those of participant observation, the better." To deal with this, let us for the moment drop the whole question of scalpel *versus* forceps, and consider one or two specific research studies, and the ways their data bear on their questions. This may allow us at least to raise what I feel is a far more fruitful set of questions: What kinds of problems are best studied through what kinds of methods; what kinds of insights and understandings seem to arise out of the analysis of different kinds of data; how can the various methods at our disposal complement one another? I can hardly attempt to contribute to the systematic discussion of these questions in a short "rebuttal"

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paper, but we can perhaps at least restate the questions in connection with some illustrative evidence.

The central problem of a recent study of the organization and internal politics of a trade union¹ was to explain the development, and especially the persistence, of a two party system within the union's political structure. To this end the research team examined a variety of documents, conducted various kinds of unstructured, focused, and highly structured interviews, examined voting records, and also engaged in participant observation.

Among the problems that we confronted was that of assessing the degree of legitimacy imputed to the party system by various groups and social categories within the union. This, I maintain, we could not have done at all adequately through participant observation. Let us leave aside the question, clearly not within the grasp of the participant observer, of whether the several hundred union officers and "leaders" were more or less inclined to think the party system a good thing in its own right as compared with the ten thousand men in a local or the one hundred thousand men in the international union we were studying. More to the point is the fact that the workings of the party system inhibited direct expressions of hostility to the system. In the ordinary give and take of conversation in the shop, party meeting, club meeting, informal gatherings after hours, such expressions were not likely to be expressed; they violated strongly held norms, and called down various kinds of punishments. It was only when we interviewed leaders individually and intensively that we could get some sense of the reservations that they held about the party system, how widely and strongly those reservations were held, and thus could make some assessment of those sentiments as a potentially disruptive force in the party system. It is true, as Becker and Geer point out, that men will do and say things in their customary activities and relationships that point to factors which might be wholly missed in the course of an interview—and where these things come to the attention of a participant observer he gains insights thereby. But the converse is also true, though perhaps not as widely recognized: Ordinary social life may well inhibit the casual expression of sentiments which are actually or potentially important elements in the explanation of the social phenomena under study. And participant observation is a relatively weak instrument for gathering data on sentiments, behaviors, relationships which are normatively proscribed by the group under observation.

I might note in passing that we gained useful insights into some of the mechanisms operating to sustain this union's political system through our observations at union meetings, party meetings, and during ordinary working days (and nights) spent in the shops. But these insights only took on full meaning in light of much other knowledge about that organization and its social and political structure that had been gained in other ways.

A recent study in the sociology of medicine—the field from which Becker and Geer draw their own illustrations—emphasizes the need for the widest variety of research methods in

attacks on comprehensive problems.² The index to the volume in which the first reports of this study are published list, under the heading "Methods of social research," the following sources of information used: diaries; documentary records; intensive interviews; observation; panel techniques; questionnaires; sociometry. Most of the papers in this volume deal with problems that could not have been studied solely through direct observation. One paper, for example, deals with the question of the processes by which medical students select their profession.³ The author finds, among other things, that the occupation of the student's father was an important element in how and when he made that decision. Becker and Geer argue that the interview is not a good source of information "about events that have occurred elsewhere and are described to us by informants." But surely certain important facts about a man's early life experience—and these include what his father did for a living—can be reported quite accurately to an interviewer or on a questionnaire, and give the analyst invaluable data for the analysis of the forces and processes involved in the choice of a profession or occupation. But the bearing of one's father's occupation, or of one's religion, on attitudes and behaviors may never emerge in the ordinary course of events which the participant observer apprehends. Moreover, it is just not true, as Becker and Geer suggest, that the interview is a reliable source of information only regarding the interviewer's conduct *during the interview*. The amount of information people can tell us, quite simply and reliably, about their past experience is very great; and it is only in light of that information, I would maintain, that we can frequently understand their behaviors in the "here and now" that the participant observer is so close to.⁴

True, if we imagine that interviews can deal with past events only through questions of the sort: "Now, why did you choose medicine as a career?" then we may indeed worry about the distortions in reporting information retrospectively. But this effort to make the respondent do the analysis for the sociologist is not the only, and almost certainly not the best, way to assess the bearing of prior events on past or current decisions.

III

We all profit, as I have from this paper, when social scientists broaden our knowledge of the special strengths of the methods which they have found useful and in the use of which they have acquired expertise. The danger lies in the kind of exclusive preoccupation with one method that leads

2. R. K. Merton, George Reader, and P. L. Kendall, eds., *The Student-Physician*, Cambridge, Mass., The Harvard University Press, 1957. See especially George Reader, "The Cornell Comprehensive Care and Teaching Program," section on "Methods," pp. 94-101.

3. Natalie Rogoff, "The Decision to Study Medicine," in Merton, Reader, and Kendall, eds., *op. cit.*, pp. 109-131.

4. This suggests, more generally, that participant observation *by itself* is most nearly satisfactory in studies of small, isolated, relatively homogeneous populations, such as primitive tribes, where variations in the character of early life experience, and the effects of those variations on present sentiments and behaviors, are not so great. Where variations in experience outside the arena being observed are great, we must, for most problems, turn to other methods of data collection to learn about them.

to a systematic neglect of the potentialities, even the essential characteristics, of another. Becker and Geer seem to display this neglect when they contrast participant observation with "the interview." But with some exceptions, the data gathered by the interviewer are not usually embodied in "the interview" taken one at a time, but in the series of interviews through which a body of comparable data has been gathered. It is all of the comparable interviews, with their analysis, that must be compared with participant observation, and not the interviews taken one at a time. The charge is frequently made, and Becker and Geer repeat it, that the interview (and especially the highly structured survey interview) is a very "crude" instrument for collecting data—its artificiality and directedness ensure that much of the "richness" of social life as it is lived passes through its meshes. I would argue that there is more than one way to gain knowledge of the richness, the subtlety and infinite variety, of social life, and that sufficiently sensitive and intensive analysis of "crude" survey data is one such way. Durkheim, whose data in his study of suicide was even "cruder" and further removed from the "rich experiential context" than that of the survey analyst, nevertheless adds much to our understanding of some of the most subtle and complex aspects of social life. How much a social scientist can add to our understanding of society, I submit, is more a product of the way he defines his problem, the questions he brings to his data, and the adequacy of his data to answer his questions and suggest new ones, than it is of how "close," in a physical sense, he gets to the social life he is studying. And this, I think, is as true for social scientists who gather most of their data through participant observation, as for those who use that method to supplement others, and for those who use it not at all.

It is no disparagement of the legitimate uses of participant observation to suggest that some of the uncritical enthusiasm and unwarranted claims for it show what seems to be a certain romantic fascination with the "subtlety and richness" of social life, and especially with "cultural esoterica," the ways very special to a given group. But it seems to me profoundly mistaken to search for the special essence of a method of data collection, and appraise it in terms of its ability to directly reflect this "subtlety and richness." As social scientists, our business is with describing and explaining social phenomena; our judgment of the usefulness of data is properly made against the criterion; how much does this help us understand the phenomenon we are studying? It may well be that participant observation is more successful than any other method in gathering data on the "cultural esoterica" of a group. But this is not a good in itself; the question remains, is this information useful, and importantly useful, for our purposes? And that of course will depend on our purposes. The correlative question is equally important: could the matters which these esoteric cultural items point to—the matters we are *really* interested in—have been learned in other, and perhaps more economical, ways? I suspect that very often they can. But at the very least the question should be raised more often than it is.

The argument the authors make for the superiority of participant observation comes finally to an expression of a preference for what can be observed "directly" over what we

must make inferences about. But the authors' strong commitment to observation leads them, I believe, to an unnecessarily dim, and basically incorrect, view of the process of inference in social science. All interpretations of data, however collected—through observations, interviews, or whatever—involve inferences regarding their meaning and significance. We confuse ourselves if we believe that the people whose behavior we are concerned with, whether we observe them or interview them, can themselves provide an adequate explanation of their own behavior. That is our job, and the participant observer makes inferences from the data he collects just as the survey analyst makes inferences from the data collected for him. The data gathered by participant observers are still data, despite the perhaps misleading circumstance that the participant observer usually both gathers and interprets the data himself, and to a large degree simultaneously.⁵ But the data he collects are not a substitute for the interpretive inference. We all forget that at our peril.

The fact that social scientists are constantly making inferences from their data does not especially disturb me, as it does Becker and Geer. Our progress in social science will come not through an effort to get "closer" to the source of data, and thus try to minimize or do away with the process of inference by dissolving it back into data collection and somehow apprehending reality directly. That simply isn't possible. Our progress will come as we are increasingly able to develop systems of theoretically related propositions—propositions which are "checked" at more and more points against data collected through a variety of means. The inferences that we make from data, and the theory from which they derive and to which they contribute, may indeed be nothing more than "educated guesses"—but that is the nature of scientific theory. Our aim is to make them increasingly highly educated guesses. We cannot evade that fate, which is the fate of science, through reliance on a wrongly conceived participant observation which apprehends social reality "directly."

IV

Every cobbler thinks leather is the only thing. Most social scientists, including the present writer, have their favorite research methods with which they are familiar and have some skill in using. And I suspect we mostly choose to investigate problems that seem vulnerable to attack through these methods. But we should at least try to be less parochial than cobblers. Let us be done with the arguments of "participant observation" *versus* interviewing—as we have largely dispensed with the arguments for psychology *versus* sociology—and get on with the business of attacking our problems with the widest array of conceptual and methodological tools that we possess and they demand. This does not preclude discussion and debate regarding the relative usefulness of different methods for the study of specific problems or types of problems. But that is very different from the assertion of the general and inherent superiority of one method over another on the basis of some intrinsic qualities it presumably possesses.

5. This involves special strengths and hazards, a matter which has been discussed extensively elsewhere, and also in the paper by Becker and Geer and their references.